

JOURNAL OF ANIMAL BEHAVIOR

VOL. 7

MARCH-APRIL

No. 2

THE DELAYED REACTION WITH SOUND AND LIGHT IN CATS

JOSEPH U. YARBROUGH

From the Psychological Laboratory of the University of Texas

I

The experiments herein reported on the delayed reaction in cats were carried out during the session 1915-16 in the Psychological Laboratory of the University of Texas under the direction of Prof. W. S. Hunter. The purpose of the work was: first, to determine the limits of the period of delay; second, to ascertain definitely the behavior during delay; and third, to describe as nearly as possible the method of reaction which leads to success. Careful records were kept of the behavior during the period of delay, and particularly of the bodily attitudes maintained and of the orientations. Associations were set up between movements that led to food and a light or buzzer, as the case might be, which could be in either of three boxes. With this association well established, tests were instituted in which the stimulus was cut off before the reaction was completed. And throughout the remaining experiments the subject had to respond in the absence of the stimulus that until now had been present at the moment of response.

It was my purpose to use in this problem a method of procedure sufficiently similar to those already used with other animals,—raccoons, rats, dogs, and children—that by comparison the relative ranking of the cat in the solution of the problem could be ascertained.

II

1. *Cats tested on light.*—The four cats used in these tests were Jim ♂, Tom ♂, Fay ♀, and Bobby ♀. Jim and Bobby were both about ten months old, vigorous, healthy animals, and their records may be accepted as typical. The other two were young cats that had not been properly cared for. They were weak and died before they were well into the experiments.

2. *Cats tested on sound.*—Four cats were used in the tests on sound. Bess ♀ and Phil ♂ were each about two years old. Judy ♀ was about one year and Kitty ♀ at least two years old. Bess and Phil continued strong and did excellent work throughout the experiments. Judy and Kitty, on the other hand, died early in the work. From this it is seen that four cats were at work practically all the time,—two on the light tests and two on the sound tests.

One would think from the number of deaths reported that the cats were in poor physical condition. Such, however, was not the case. Their general health was very good. Those that died did not experience a long period of sickness, but died within thirty-six hours of the appearance of distress. There was only one exception to this, and in this case the cat was replaced by another rather than risk her recovery.

It was much more difficult than I had expected for them to become physically adjusted to their new environment. They were kept in a wire cage 12' by 3½' by 6' high, in a room adjacent to the experiment room. Their room was well ventilated, and a large east window provided an inlet to the morning sunshine. The difficult thing was to find the most nourishing food for them. Milk, with a small amount of raw steak, proved to be the most satisfactory.

III

DESCRIPTION OF APPARATUS AND METHODS

In Fig. 1 is shown the ground plan of the box used. The box was made of ¼" boards and was 26" high, with the doors at a, b, c, 10" high by 7" wide. The distance between these doors was respectively 20", and the distance from the release door E to each of the doors was 44". The door E of the release box was raised by a cord passed over a pulley directly above it and 6½' above the floor of the apparatus. Besides this pulley

were three other pulleys through which passed cords from the three sliding doors marked D in the figure. With the aid of these cords, the experimenter could stand behind the release box and control the door at each of the boxes. The release box was covered with wire of $\frac{1}{2}$ " mesh, and the board B upon which was fastened the switches for both light and sound. The light stimulus came from 8 c.p. lamps, so wired that any one of them could be cut in at a time. The current was obtained from a 110 volt switchboard B.

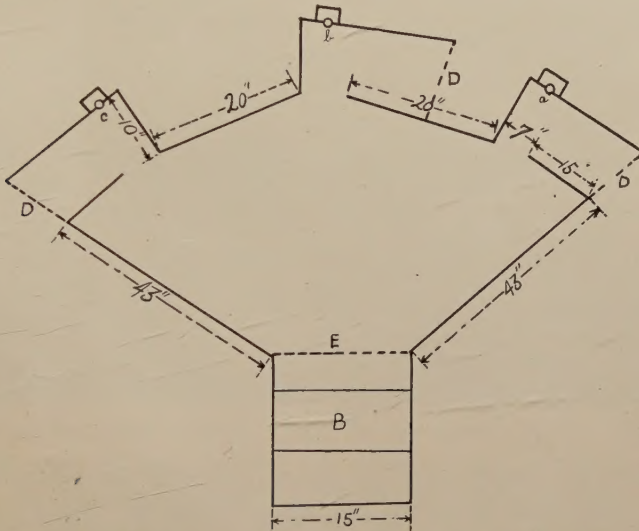


FIGURE 1.—Ground plan of apparatus

In order that the cat might not come in contact with the lamps, and, also, not be hindered in entering the boxes, a hole was bored in the back wall of each of the boxes and a lamp placed outside and behind each box. The holes were of the same size and 5" from the floor. The lamps were mounted on bases which rested on the floor, and were placed behind the holes so that they had equal intensities and could be observed with equal ease from the release box. One 8 c.p. lamp hung over the center of the apparatus and 4' from the floor throughout the experiment. This light was shaded with a paper bag which made it necessary to keep fresh sawdust on the floor of the box to make the movements of the animals clearly visible. At the

outset I was compelled to cover the entire apparatus, as the cats were free to jump out at will. The wire used for this purpose was of $\frac{1}{2}$ " mesh and its tendency to blur the field of vision made it still more necessary that the white sawdust be used.

Fig. 2 should give a clear presentation of the essentials of the box when taken in connection with Fig. 1.

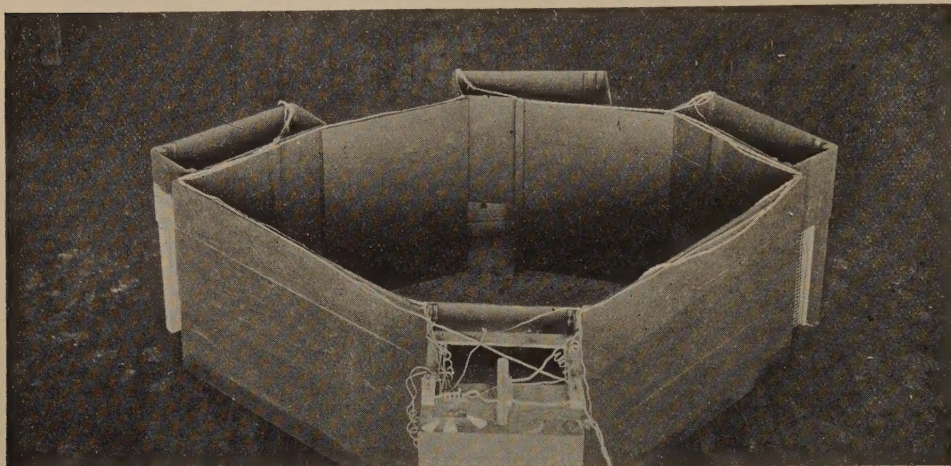


FIGURE 2

The cats on sound used the same apparatus as those on light, the only difference being the change in stimulus. On the switchboard B, Fig. 1, were placed three buttons which corresponded to each of the three light boxes, a, b, and c. In each of these boxes a buzzer was suspended directly over the door and 12" from the floor of the apparatus. These buzzers were suspended by a coiled wire, and were not in contact with the apparatus. The system of wiring was the same as that of lighting—i.e., any buzzer could be sounded at the wish of the experimenter by pushing the proper button on the switchboard B. Such an arrangement made it possible for the experiments on both sound and light to be carried on without any interference. So far as the knowledge of the experimenter goes, the cats on light never found the buzzers, nor did the cats on sound find the lamps.

The general method of experimentation was as follows: The animal to be tested was put in the release box which is

shown in Fig. 1. Now, suppose, for example, that the lighted box were the one on the left, c; its exit door would be opened and its light turned on. When the experimenter was sure that the cat had seen the light or heard the sound, the animal was released. A careful, detailed record was kept of the direction in which the animal was oriented at the moment of release and its path to the exit. Any unusually wide turn in the path was always recorded. Hesitation and zig-zag movements were especially noted whenever and wherever they appeared in the cat's response. In these experiments the cats should go straight to the lighted box, and through its exit door and back to the entrance of the release box where they got food. With the cats on the sound problem, the reactions were the same. With them, however, the "lighted" box was a "sound" box.

When the cats were sufficiently trained to choose the stimulus box (lighted or sounded, as the case may be) almost perfectly, delays were begun. The periods of delay were much the same as those used by Hunter.¹ The first delay was to turn the stimulus off just as the animal reached the box. In the second delay, the stimulus was cut off when the animal was half way to the box. In the third delay, the stimulus was stopped just as the animal made its first move in response after the door of the release box was raised. And, in the fourth delay, the stimulus was cut off just before the door of the release box was raised. In this fourth stage a genuine delay first enters in. The first three stages of delay were of little or no value as delays. Their primary purpose was to bridge over the period of stimulus to non-stimulus, to bridge that period between acting in the presence of a stimulus and acting in the absence of a stimulus. All that was necessary to make a correct response was, in each case, for the cat to continue in the direction he was going. There was no further choice to be made. The fourth delay, however, was genuine, although of small duration. *The stimulus was cut off before the cats were released.* Throughout the remainder of the experiments the cats were compelled to react in the absence of the stimulus that until now had been present at the moment of response.

There was no definite standard adopted by which to promote

¹ Hunter, Walter S. The delayed reaction in animals and children. *Behav. Mon.*, vol. 2, no. 1, 1913.

from one period of delay to another. The general method used, however, was to promote the animal as fast as possible, and only demote when the records showed him unable to bridge the delay. There were no special arrangements made for punishment in case of error. It was easy to observe, however, that there was a certain degree of punishment following each error. These punishments were: having to back out of a box, and having food and freedom deferred for a longer period of time. Although the cats were expected to go straight to the stimulus box, no wide turn in their pathway is recorded wrong unless they approached the entrance to one of the other boxes. The apparatus was so constructed that the animal could not see the position of an exit door, i.e., whether it was open or closed, without actually approaching the particular box.

IV

EXPERIMENTAL RESULTS

1. THREE COMPARTMENT EXPERIMENTS

A. *Learning the association.*—Although the primary purpose of this investigation is a study of the delayed reaction proper, it is well to make additional note of the learning process. Table I gives the number of trials required by the cats of set A to learn the association between the light and the getting of food. Each cat was given 10 trials daily. Fay, the last reported in the table, died at the end of 50 trials. Her results are reported, however, because 75% of her last 20 trials were successful.

TABLE I

Cat	CATS TESTED ON LIGHT				Per cent correct of last 50
	Number of trials	Number correct	Per cent correct	Number correct of last 50	
Bobby.....	130	96	73	47	94
Jim.....	110	84	76	49	99
Tom.....	170	112	65	45	90
Fay.....	50	25	50	25	50

The number of trials required by the cats on sound, set B, are given in table II. The last cat reported in this table died at the end of 40 trials. For this reason no record of her work appears in the last two columns of the table.

TABLE II

Cat	CATS TESTED ON SOUND				
	Number of trials	Number correct	Per cent correct	Number correct of last 50	Per cent correct of last 50
Bess.....	180	123	68	43	86
Phil.....	70	44	63	37	74
Kitty.....	110	68	61	32	64
Judy.....	40	26	65

These results indicate, first, that the cats of each set learned the association readily. The learning curve would appear short and steep. And, second, they indicate that it is more difficult to maintain a high efficiency in set B than in set A. This is indicated by the fact that, while Bess and Phil, both of set B, made 86% and 74% respectively correct in the last 50 trials, Bobby and J m, of set A, made 94% and 99%.

Although the differences of results, as given in the tables above, are not conclusive, the experimenter is of the opinion that the sound tests present the more difficult problem. These variations may well be explained on the basis of individual differences, but it is to be noted that the animals tested on light have the better records. This probable increase in difficulty in the sound tests is due, no doubt, to the timidity on the part of the cats when approaching the sound. The records show, as is indicated in the next paragraph, that the cats were for some time rather frightened by the sound of the buzzers. This caused an increase in the number of errors and so a decrease in the percentage of correct reactions. Before a definite conclusion can be reached a sufficient number of cats, to eliminate errors from individual variations, must be tested.

Observations of the behavior of the animals during the learning period on sound, which were recorded from day to day, suggest several smaller divisions. (1) A period of disregard. My notes read, "Bess appears to give no attention to the buzzer," and, again, the next day, "Bess walks about freely without noticing the buzzer." (2) A period of disturbance. This period may be characterized by a behavior which may be termed "awareness" or "worry." The cat stops, turns head, looks, and calls as if in danger. This note is recorded, "Bess dislikes to go to the sound. She appears shy and afraid of the buzzer. She will venture to the door, stop, and squat; look up at the buz-

zer and sometimes rise up and 'sniff' at it before going into the box." (3) A period of hesitation. The behavior of this period is characterized by wavering and by starts and stops. And, in period (4), the cat gives strict attention to the stimuli. Here the behavior becomes more nearly perfect, the path of reaction has been made straight, and the percentage of correct reaction is high. With the animals tested on light, set A, the same learning period divisions could be made. In this case, however, the period of disturbance was not accompanied by so much timidity and fear.

During this period of experimentation all possible care was taken to prevent any preference for particular boxes. Should such a tendency be observed, control tests were given to break it up before the position habit was well developed. At the end of the first 60 trials each box had been presented 20 times, and the records show that no box was chosen more than 26, nor less than 16 times by any one of the eight subjects.

For comparison we bring together in table III data on learning the association obtained by Hunter in his study of animals and place beside it that of our own subjects.

It is of interest to note that all the cats fall in the class with Bob, Hunter's most rapid raccoon. Bob learned the association in 120 trials while the eight cats used in these tests ranged from 50 to 180 trials with an average of 107 trials each. The curve representing the learning period for the discrimination of the three compartments was very short and steep, yet broken and irregular. With continued practice, this irregularity would undoubtedly have been eliminated; and the cats of each set would have attained perfect mastery of their problem.

TABLE III

	Number of trials on learning		Number of trials on learning
Raccoons—		Rats—	
Bob.....	120	No. 9.....	280
Betty.....	340	No. 12.....	440
Jack.....	540	No. 13.....	250
Jill.....	825	No. 15.....	220
		No. 16.....	480
Dogs—			
Blackie.....	560		
Brownie.....	650		

TABLE III—*Continued*

	Number of trials on learning	Cats—	Number of trials on learning
Rats—		Bobby.....	130
No. 2.....	176	Jim.....	110
No. 4.....	175	Tom.....	170
No. 5.....	505	Fay.....	50
No. 6.....	800	Bess.....	180
No. 7.....	361	Phil.....	70
		Kitty.....	110

(c) *Controls used.*—In the construction of the apparatus, every effort was made to eliminate all possible differences in the compartments which could be used as guides to correct reactions. The backgrounds surrounding the entrances to the compartments were all alike painted black. Since the backgrounds were all of the same brightness, and, since everything remained constant with the single exception of the exit doors to the compartments, controls were put in to determine their possible effect. In order to test this, the three doors were all opened and the tests were given by the usual method under conditions in all other respects normal. The results were entirely negative. In no case did an animal make use of the doors as cues to its reactions.

Again, control tests were introduced to determine whether or not the animals were really depending upon the applied stimuli (light or sound) for cues for guiding their reactions. To test this, experiments were made under normal conditions except that each time the stimulus (sound or light) was withheld .30% correct reactions was the highest made by any subject under these conditions. It is clear, therefore, that normally the reactions were made either to sound or to light.

Not being able to secure the same pitch and intensity in each of the three buzzers, control tests were made to determine whether the animals had formed associations between them on the basis of quality. The buzzers were all interchanged—buzzer *a* took the place of *b*, *b* the place of *c*, and *c* the place of *a*. No case was found where the differences in pitch and intensity were used as cues for reaction. These qualitative differences could well have been effective during the period of learning the association; but, on the delayed experiments, they could be of little or no value. The essential cues in handling delays must be factors

that are variable from trial to trial otherwise they cannot be selective in nature.

No temperature controls were used. They were thought to be unnecessary because of the following: 1. The lights were turned on but for a short time. 2. They were outside of the main apparatus. 3. The cats oriented immediately when the lights were turned on and reacted precipitately when released. And, 4, the behavior of the cats on light was the same as that of those on sound where temperature could not be involved.

B. "*Delayed*" experiments.—Since in the first four delays used the entire reaction was not performed after the stimulus had been removed, it is probable that they should not be termed delays at all. The stimulus was always continued until the experimenter was convinced from all external evidence that the cat had become aware of its presence.

The cats tested on sound and those on light were all presented their problems by the method described above, but for convenience the data will be discussed separately.

(a) *Set A (cats tested on light).*—

Delay I.—In delay I the light was turned off just as the cat reached the correct compartment. Bobby was given 30, Jim, 20 trials; and for both of them each trial was successful. With the association well established, the turning off of the stimulus at this point in the reaction effects no change in their percentage of correct response.

Delay II.—In this delay the stimulus was cut off when the cat was half way from the release box to the correct compartment.² Jim was given 10 trials with all of them correct. Bobby was given 60 trials with 56 correct. There appears to be no difficulty in making the step from delay I to delay II, even though the cats here made one-half of the distance of response in the absence of the stimulus. After the cat is well set out, then, on his reaction, the stimulus may be withdrawn without affecting the response.

Delay III.—The only difference in this delay and number II is that here the stimulus is withdrawn before the cat is well on

² In case the cat started from the release box in a different direction from that of the stimulus, e.g., if he started toward c when the stimulus was at a, the stimulus was not turned off until he did turn in the direction of the stimulus compartment, and so in this case, was well on his way.

his way, while in II the reaction was half completed. Jim was given 20 trials all of which were correct. Bobby was given 60 trials with 57 correct. The reader will notice that the cats have still met no difficulty.

Delay IV.—Bobby was given 80 trials of which 66, or 82%, were correct. Jim received 130 trials, 107 of which were correct, making also 82%. Here the first difficulty of bridging over a period of delay appears. The door of the release box and the cutting off of the stimulus were operated simultaneously and without reference to where the cat was or what it was doing. Thus the animal was forced to initiate the reaction and perhaps make a choice of compartments, in the absence of the stimulus. The data show that Jim took much longer to master this delay than did Bobby, although he had held a higher percentage on fewer trials in the three preceding delays. The fact that Jim had received 100 trials less than Bobby in these preceding delays can be offered as explanation of his need of 60 more trials here. It seems natural that had he not been advanced so rapidly from one delay to another he would have been better prepared for this new delay, and, being better prepared, would have bridged over it much more quickly.

Two seconds delay.—At this point in the experiments a metronome was placed in an adjacent room to mark the period of delay in seconds. At this distance its sounds could be easily heard by the experimenter, yet they were not thought to be strong enough to distract the attention of the animals.

Bobby was given 130 trials with 106, or 81% correct; while Jim was given 200 trials, 143, or 71% of which were correct. Of the last 40% of Bobbie's trials, 34, or 85% were correct; of the last 40% of Jim's trials, 32, or 80% were correct. The data do not show that the reactions were poor at the beginning of the delay and grew better with successive trials, but rather show an irregularity throughout. Bobby, e.g., was perfect on the first 10 trials, while after having received 70 trials she made only 50% on 10 trials. Again, Jim, after 160 trials, made only 30% on 10 trials, yet on the 10 just preceding, he made 90% and 80% on the 10 immediately following.

Four seconds delay.—In the four seconds delay experiment, 180 trials were given Bobby with 141 correct, and 150 given Jim with 118 correct. Each made 78% correct. Jim's last 30

trials showed much improvement, 29 of them being correct reactions. Although Bobby had 30 more trials on this delay than Jim, she made only 26 out of her last 30 trials. This is readily explained in the light of the fact that the middle compartment was dropped out during this period with Jim, while Bobby continued on three compartments. Jim had made 70% on the last 40 trials preceding the 30 trials mentioned above, of which he got 29 correct. At the end of these 40 trials, the middle compartment was dropped out. Of the first 10 trials with only two compartments, Jim made 100% correct. The records show that Jim would have made a higher percentage than 70 much sooner had it not been for a tendency to drop out the middle box. This is not only seen in table IV, but by the fact that when he received the stimulus only from boxes "a" and "c" (the experimenter having dropped out the middle box), he made 100% on the first 10 trials.

TABLE IV
DAILY RECORD ON 4 SECONDS DELAY WITH LIGHT

Cat	Number of trials	Number correct	Distribution of errors		
			a	b	c
Bobbie.....	10	6	1	2	1
	10	9	0	1	0
	10	10	0	0	0
	10	7	2	0	1
	10	7	2	0	1
	10	8	0	2	0
	10	8	0	2	0
	10	7	1	1	1
	10	8	0	2	0
	10	8	0	1	1
	10	8	0	2	0
	10	7	1	2	0
	10	7	1	1	1
	10	8	0	2	0
	10	8	1	1	0
	10	9	1	0	0
	10	9	0	0	1
	10	9	1	0	0
			11	19	7
Jim.....	10	9	0	1	0
	10	8	0	2	0
	10	7	0	2	1
	10	6	0	3	1
	10	7	0	3	0
	10	7	0	2	1
	10	8	0	2	0

TABLE IV—*Continued*

Cat	Number of trials	Number correct	Distribution of errors		
			a	b	c
Jim.....	10	9	0	1	0
	10	7	0	3	0
	10	7	0	3	0
	10	7	0	3	0
	10	*7	0	3	0
	10	10	0	0	0
	10	9	1	0	0
	10	10	0	0	0
			1	28	3

Six seconds delay.—Bobby was the only cat either on light or sound that was tested on the six seconds delay before the middle box was taken out. Although she had made 85% on her last 40 trials on the four seconds delay, she fell to 50% on the first 10 trials in the six seconds delay. After 90 trials with only 50% of the last 40 correct, she was put back on the four seconds delay where she was given 70 trials, making 80% on the last 50. She was again given 20 trials on the six seconds delay with 55% correct. After 60 more trials on the four seconds delay with 85% correct, she was given 20 trials on the six seconds delay with 70% correct. The records show that during the six seconds delay she became restless and often turned around in the release box. In such cases she usually went to boxes *a* or *c*, depending upon the one she *came in line with first* in making a circle by her turning in the release box. It is the writer's opinion that with further training cats can bridge the six seconds delay with three boxes.

(b) *Set B (cats tested on sound).*—

Delay I.—The two cats that continued the work on sound after the death of their fellows were Bess and Phil. Bess was given 60 trials, 48 of which were correct. Of the last 30 trials, 26 or 86% were correct. Phil received 30 trials with 25 or 83% correct, and 9 of the last 10 correct. As in the case of the light experiments, no difficulty was encountered by cutting off the stimulus at this point in the reaction.

Delay II.—One hundred trials were given Bess, and she made 80% correct reactions. Phil received 50 trials which contained 90% correct. Of the last 20 trials 19 were correct. Although

* Middle box was dropped out here.

the cats were making the last half of the response in the absence of the stimulus, no difficulty yet appeared.

Delay III.—On this type of delay, where the stimulus was cut off the moment the cat left the release box, Bess was given 100 trials. Of this number 84 were correct, with 54 of the last 60 correct. Phil made 56 correct reactions out of 70 trials, making a percentage of 80. These results mean that, having started correctly, the cats are able to retain their cue for correct reaction even though the remainder of the reaction must be made in the absence of the stimulus.

Delay IV.—In this delay, Bess was given 60 trials, 55 of which were correct, and, of the last 40 trials, 39 were correct. Phil was given 60 trials with only 60% correct. Of the last 30 presentations, 18 were reacted to correctly. Table V shows Phil's tendency to drop out compartment b whenever the delays set in.

TABLE V
DAILY RECORD ON DELAY 4 WITH SOUND

Cat	Number of trials	Number correct	Distribution of errors		
			a	b	c
Bess.....	10	10	0	0	0
	10	6	2	2	0
	10	10	0	0	0
	10	9	0	1	0
	10	10	0	0	0
	10	10	0	0	0
			3	3	0
Phil.....	10	7	0	2	1
	10	7	0	3	0
	10	6	1	2	1
	10	4	0	4	2
	10	7	0	2	1
	10	7	0	3	0
	*10	8	0	2	0
	10	9	0	1	0
	10	8	1	1	0
	10	7	0	3	0
			2	22	5

Two seconds delay.—On this two seconds delay, Bess was given 130 trials and of this number 116 or 89% were correct. Of the last 80 trials, 74 were correct; with the percentage of 92, she was promoted to the four seconds delay. Phil was given

* From January 23rd to February 3rd, Phil was being retrained on delays II and III, receiving 10 trials each day.

60 trials with 51 or 85% correct. Of his last 40 trials, 35 were correct.

Four seconds delay.—One hundred and seventy trials were given Bess with 121 correct responses. Of the last 30, only 18 were correct. With this low record, it was thought best to return to shorter delays before trying to advance. From January 24th, 1916, until February 14th, she was given 10 trials daily on delay IV and on two seconds delays. Of the 200 trials given during this period 120 were given on the two seconds delay, the last 30 of which netted 28 correct reactions. This high percentage of correct reaction on the last 30 is due to the dropping out of the middle box; so, also, may the low percentage of correct reaction immediately preceding be explained by the tendency to drop out the middle boxes the delays were lengthened. The percentage of correct responses in the last 30 trials immediately preceding the dropping out of the middle box was 66, while the percentage of the first 30 after its being dropped out was 94.

(c) *Maximal interval of delay with three boxes.*—

In table VI the maximal delays attained by my cats are given, and for comparative purposes similar data on Hunter's animals and Walton's dogs are included. The reader should remember that these tests were made with a choice of three boxes and that training stopped here because of a well developed tendency to drop out the middle box. In the case of Phil, the last cat reported in the table, this tendency was not well developed. As is shown in the table, he was making a good record on the two seconds delay, and there are no indications that he could not have bridged a longer period of delay with three boxes.

TABLE VI

	Animal	Maximal delay	Number of trials	Per cent correct
Hunter.....	Rats—			
	No. 13.....	4 secs.	...	88
	No. 15.....	1 sec.	...	86
	No. 16.....	1 sec.	...	50
	No. 17.....	7 secs.	...	68
	Dogs—			
	Blackie.....	5 mins.	...	80
	Brownie.....	2 secs.	...	68

TABLE VI—*Continued*

Animal	Maximal delay	Number of trials	Per cent correct
Raccoons—			
Jill.....	3 secs.	...	93
Jack.....	20 secs.	...	85
Bob.....	25 secs.	...	90
Walton.....	10 secs.	...	64
Dogs—			
Present work..Cats—			
Set A (light) Bobby	4 secs.	160	85
Jim...	4 secs.	120	78
Set B (sound) Bess.	4 secs.	170	71
Phil.	2 secs.	40	83

It will be noted that the longest delay mastered by the cats was a period of four seconds. I am sure that with continued training they can bridge a much longer period than this. But, since I was more interested in the behavior during delay than in the maximum period of delay; and since at this point there had developed a tendency to drop out the middle box; and, again, since time was limited, I thought it best not to give further training on three boxes.

(d) *Longer delays.*—

Scattered throughout the experiments are correct reactions over periods of delay much longer than those mastered in the regular series. These periods were willingly lengthened by the subjects themselves. This in itself is good evidence that with sufficient training a much longer interval of delay could be mastered. At three different times Bess made 9 correct reactions out of 10 trials with a delay period of six seconds. And, at another time she made, with the same interval of delay, 17 correct responses out of 20 trials. Twice she responded correctly after a delay period of twenty-six seconds. It will be recalled that Bess was tested on sound. Jim, also tested on sound, bridged at one time a period of eight seconds, at another a period of eighteen seconds, and a third of thirty-four seconds. The cats on light seemed not to have hesitated so often as did those on sound. In all the work on the three box experiments, Bobby was the only cat tested on light who voluntarily lengthened her period of delay. On this occasion she sat for sixty-six seconds in the release box, after which she went directly to the proper compartment. All the periods of hesitation were not measured and tabulated. Animals, both of Set A and Set B,

hesitated often from one to three seconds on a single reaction, but their occurrence was so irregular and their duration so brief that their measurement and tabulation were very difficult. Therefore, no period was recorded in seconds unless it was of considerable duration. However, all hesitations were entered in the notes.

2. TWO COMPARTMENT EXPERIMENTS

A. "*Delayed*" experiments.—The delay work was continued in the two compartment tests by the usual method. The series of presentations of boxes was changed from

ab	cc	ba	ba	cb
bb	ca	ca	bc	ca
ba	ca	bb	ca	ac

One of these three series of ten had been used each night. Each one was used an equal number of times and at no time was one given twice in succession. In this way no one series was given twice within three days. On the two compartment tests the number of series was increased to four, as follows:

ac	ca	ac	ca	ca
ca	ac	ca	ca	ac
aa	ca	ca	ac	ac
cc	aa	ca	ac	ca

These were taken in their order beginning with the first, and no one was, therefore, given twice within four days.

(a) *Cats tested on light*.—It will be remembered that in table IV Jim is reported to have made 29 out of the last 30 trials correct, after a four seconds delay. As his work progresses on the two compartment experiments, the period of delay increases. Since a very large proportion of his errors on the three compartment experiments were due to a tendency to drop out the middle box, he would be expected to make a higher percentage of correct reaction with this box omitted. Such is shown to be the case in the data below.

Of the 40 trials given Jim on six seconds delay 34 were correct. He escaped from the laboratory on the third day, after his work, and after 36 hours absence was recovered and made 80% on 10 trials. Feeling sure that the cat was experiencing no difficulty, the experimenter increased the period of delay to

eight seconds. Jim was given 30 trials with this period of delay 27 of which were correct. As he appeared to meet no difficulty in bridging this period, he was set to work on ten seconds delay where he reacted 28 times correctly in 30 trials. On twelve seconds delay he made 90% on 30 trials. Since he had so successfully bridged over these small advances in delays, the next increase was double in length, i.e., four seconds. Forty trials were given with a delay of sixteen seconds. The problem did not seem to increase in difficulty for 36 of these 40 presentations were reacted to correctly.

The longest period of delay in which a regular series of experiments were offered was eighteen seconds. One hundred tests were given Jim on this period of delay, and of this number he responded correctly to 91. During these experiments, Jim was observed as closely as possible as to the orientation of head and body when the door of the release box went up, and also at the moment he initiated the movement of response. The matter of orientation will be taken up again under the discussion of "behavior during delay."

(b) *Cats tested on sound.*—Bess and Phil had been dropped back to the two seconds delay before the middle box was dropped out. Beginning with the two seconds delay they were promoted simultaneously from one interval of delay to another.

Figured on the basis of 30 trials, Bess' percentage jumped from 66 to 95, and Phil's from 80 to 96. On the four seconds delay no difficulty was met. After 40 trials,—Bess with 93% and Phil with 99,—they were promoted to the six seconds delay. Here they received 40 trials, Bess making 95%, while Phil made only 77%. This low percentage on the part of Phil was caused by a pronounced position habit which appeared on the first day and lasted through the second day of the series. They each received 30 trials on both the eight and the ten seconds delays, and each held a percentage of about 85. Since these periods were bridged so easily, the period of delay was now lengthened to fourteen seconds. On this interval of delay, 40 trials were given, and Bess held 87%, while Phil made 98%. Ninety trials were made by each cat on the sixteen seconds delay. Of these 90 trials, Bess was successful 84 times, and Phil 81 times. During this last period of delay of 90 trials, special observation was made of orientation. These observations were recorded in detail,

and will be carefully considered under "behavior during delay and after release."

(c) *Maximal interval of delay attained with two boxes.*—Table

TABLE VII

	Animal	Maximal delay	Number of trials	Per cent correct
Hunter.....	Rats—			
	No. 4.....	1 sec.	20	75
	No. 11.....	5 secs.	70	81
	No. 15.....	5 secs.	60	67
	No. 16.....	5 secs.	50	90
	Dog—			
	Blackie.....	3 mins.	30	86
	Raccoons—			
	Jack.....	20 secs.	40	85
	Betty.....	10 secs.	30	86
	Bob.....	25 secs.	20	90
Walton.....	Dogs.....	1 min.	10	80
Present work..	Cats—			
	Set A—Jim.....	18 secs.	90	90
	Set B—Bess.....	16 secs.	90	93
	Phil.....	16 secs.	90	90

VII gives the maximal delay attained on two boxes by the subjects studied by Hunter, Walton's dogs, and the cats of the present experiments. The cats rank very well with Hunter's raccoons in successfully bridging delays with two boxes. Just what interval of delay could finally be bridged with the two box tests is not known. It is evident from the above table that the limit of the cats' ability was not reached. I see no reason why the interval may not be increased even into minutes.

This opinion is based upon the fact that the records show many reactions where the period of delay is of much longer duration than eighteen seconds, the greatest recorded in the above table. The following long periods of delay were each followed by successful reaction. Phil lengthened his delay period twice during this period of 90 trials, once to twenty seconds and once to twenty-two seconds. Jim reacted correctly after three such periods of delay, twenty-four seconds, twenty-six seconds, and thirty seconds. Bess was successful after the following delays: 1 twenty seconds duration, 3 twenty-two seconds, 1 twenty-four, 1 twenty-six, 1 thirty-two, 2 thirty-six, 1 forty-two, and 1 fifty-two seconds duration.

It will be noted that all animals delayed much longer with

two than with three boxes. This is readily explained on the basis of the relative complexity of the problems, and the effect of continued training.

3. BEHAVIOR DURING DELAY AND AFTER RELEASE

Four different types of behavior appeared in our experiments: (1) The animal maintained an orientation of all its body during the interval of delay, i.e., it kept both its head and body pointing toward the proper box; (2) the animal kept either its head or its body in perfect orientation; (3) no observable part of the animal's body was retained in constant position, i.e., the experimenter could detect no orientation cues used by the animal, (4) the animal held some certain position in the box, i.e., it actually went to the point in the release box nearest to the proper compartment and there awaited to be released. Types 1, 2, and 3 will be combined for convenience in the discussion, and will be followed by a consideration of 4.

A. Orientation of whole or part of body.—Great pains were taken to insure accuracy in the recording of orientations. Records were kept not only of the body position, but of whether *any observable part* of the animal remained in a constant position during the delay period. Also note was made of any case where the animal turned partly or entirely around, as well as of the direction in which it turned. In order to obtain as accurate data as possible on orientation, a series of 300 tests were specially given where the orientations of both the head and body were recorded *at the moment the door of the release box went up, and again when the animal made its first motion to leave the box*. Tables VIII and IX give a summary of these reactions showing just what orientations preceded them. In the first table only those tests are recorded where the orientation was *different* when the animal *started* from what it was when the *door went up*. While in the second table all tests are recorded where the orientation was *the same* when the animal started as it was when the release door went up.

TABLE VIII

When door went up:	Correct	Wrong
Good orientation of head only.....	108	1
Good orientation of body only.....	30	1
Good orientation of head and body.....	118	0
Poor orientation of head and body.....	18	24

TABLE VIII—*Continued*

When animal started:	Correct	Wrong
Good orientation of head only.....	2	0
Good orientation of body only.....	0	1
Good orientation of body and head.....	259	3
Poor orientation of body and head.....	9	26

TABLE IX

	Good	Bad
Good orientation lost <i>between</i> release and starting.....	107	1
Good orientation not lost between release and starting..	40	0
Poor orientation at release and at starting.....	4	12

This table indicates plainly the similarity of the behavior of my cats and Hunter's rats and dogs. The cats almost never reacted in opposition to their orientation. (Here I mean, of course, the orientation of both head and body, for many times they reacted correctly in accordance with only the head or the body.) Of 141 errors made by one of Hunter's dogs, 116 were preceded by faulty orientation. So, also, the cats' errors, as the table shows, were in almost every case preceded by faulty orientation. The non-orientation reactions are few enough to be accounted for by chance.

B. Position in the box.—Owing to the fact that during the period of long delays only two boxes were used and they were located far apart, the cats could have shifted their behavior from the use of *orientation cues* to the use of position cues. Information on this point was hard to get: (1) Because of the continuous movements of the animals, and (2) because the size of the release box in comparison with the distance to the exit box is so small that but little is gained by being at one side or the other. However, from the few observations made, the writer is of the opinion that the position of the animal in the release box does aid its reaction.

4. REACTION TENDENCIES

In order to get representative data on errors and position habits and the frequency with which these stereotyped forms of response interfered with the work, I shall present 610 of Jim's and 630 of Bess' reactions. It will be remembered that Jim was tested on light and Bess on sound. The first 320 of Jim's 610 reactions were made on the three box experiments, while the remaining 290 were made with only two. Position habits in which one particular box was always chosen first, occurred

with each animal on each of the three boxes. Now one box was chosen first and now another. For convenience these data will be recorded in two separate tables (X and XI), the first containing data recorded on the three box experiments, and the second, those obtained when only two boxes were used.

TABLE X
THREE BOX EXPERIMENTS

Order of response.....	abc	acb	ab	ac	Total reactions made
Jim.....	7	3	22	1	33
Bess.....	7	1	22	4	34
Order of response.....	bac	bca	ba	bc	
Jim.....	0	1	0	1	2
Bess.....	2	0	7	3	12
Order of response.....	cab	cba	ca	cb	
Jim.....	8	6	3	23	40
Bess.....	0	3	4	15	22

Table X analyzes all incorrect responses made on the three box experiments, here included, and gives the relative number of times each subject followed the different possible orders. The number of errors made beginning with boxes *a* and *c* is about equal with both animals, while the number beginning with *b* is very low. In fact, as the table shows, Jim only made 2 errors when *b* was selected first. Bess, however, made 12 such errors, or about one-half the number she made after selecting *c* first. Of the 40 times Jim selected *c* first, he selected *b* next 29, or 72% of the time. And of the 33 times he selected *a* first, 29, or 87% of the time *b* was the next box selected. When *a* was selected first by Bess, she chose *b* next 29 times out of 34, or 85% of the time. And when she selected *c* first, she chose *b* next 18 times out of 22, or 81% of the time. It may be concluded then, that whenever the reaction began at the end of the row of boxes, i.e., *a* or *c*, the tendency was to take the boxes in order until the solution was reached. Only three times in 320 trials did Jim go to the same box twice in the same trial. (These are listed in table XII as "persistent errors.") The form of this position habit was *c b c b a*, and was repeated three times within 20 trials. Bess returned to the same box in the same trial only one time, and the order of the boxes chosen was *b a b c*. One further thing to be noted is that Jim made 25 "three place errors," responses where the animal tests each

of the three boxes before the solution is reached. This type of error was made 13 times by Bess. This form of behavior is apparently less frequent than in Hunter's child³ and far less frequent than in Hamilton's dog.⁴

TABLE XI
TWO BOX EXPERIMENTS*

Order of response.....	abc	ac	bac	ba	Total reactions made
Jim.....	0	14	0	0	14
Bess.....	6	14	0	0	20
Order of response.....	bca	bc	cba	ca	
Jim.....	0	0	0	15	15
Bess.....	0	0	0	6	6

* Since exit *b* is no longer open, all orders of choice ending in *b* are omitted in this table.

Table XI contains a record of the errors made in the two box experiments. Jim had so completely lost the cue to *b* that not one time after that box was dropped out did he return to it. Although Bess never made *b* her first choice again, she at six different times on her way from *a* over to *c* stopped by and examined *b*. It will be noticed that the number of errors greatly decreased with the elimination of the middle box. This may be accounted for, first, by the increase in the simplicity of the problem and, second, by practice.

It would be well worth while to put beside the reaction tendencies of these two cats similar data gathered by Hunter on rats, raccoons, dogs, and children. Table XII gives a summary of the errors made by his subjects, and includes, also, those for my two cats. Some explanation of this table is necessary.

TABLE XII

Animal	Number of trials	Total number of errors A	Three place errors B	Persis- tent errors C	Per cent of A to B	Per cent of B to C
Child.....	264	120	54	6	44	11
Raccoon—Bob.....	720	209	78	29	32	37
Dog—Blackie.....	570	127	75	25	59	33
Rat—No. 9.....	575	144	42	13	29	30
No. 2.....	345	152	69	47	45	68
Cat—Jim.....	320	75	25	3	33	12
Bess.....	330	68	13	1	19	7

³ Hunter, W. S. The delayed reaction in a child. *Psych. Rev.*, 1917, vol. 24, 74-87.

⁴ Hamilton, G. V. T. An experimental study of an unusual type of reaction in a dog. *Jour. Comp. Neur. Psy.*, 1907, 17, 329-341.

The raccoon's records include delays from one second through twenty seconds; those for the dog, from one second through seven seconds; those for rat No. 9, from the third stage of delay (turning light off just as the animal was released) through seven seconds; those for rat No. 2, from the third stage of the delay through one second; and those for Jim and Bess, from one second through four seconds. The data are compiled here for comparative purposes and will be easily interpreted without further comment.

Available data at the time of the preparation of Hunter's paper on the delayed reaction in a child made it clear that there were no marked differences between animals in the reaction tendencies displayed under the experimental conditions in question. It did look, however, as though there were marked differences between the animals and the child. Our data here presented place the cats in a class with the child. So far then as this type of test is concerned, no essential differences between man and other animals have been brought to light.

CONCLUSIONS

1. All the cats herein tested learned the initial association within from 100 to 180 trials and therefore fall into a class with Hunter's raccoons, so far as rapidity of learning is concerned.
2. No differences of method in solution of delays was observed between cats on light and those on sound.
3. The minimum and maximum delays were two seconds and four seconds on the three compartment experiments; while with only two compartments, they increased to sixteen seconds and eighteen seconds respectively.
4. The cats solved the problem by maintaining gross motor attitudes of the whole or part of the body.

TEMPERAMENTAL DIFFERENCES BETWEEN OUTBRED AND INBRED STRAINS OF THE ALBINO RAT

NENOZO UTSURIKAWA

From the Harvard Psychological Laboratory

INTRODUCTION

About two years ago the writer sought, in the Harvard Psychological Laboratory, training in the methods of comparative psychology, since such training promised to be helpful to him as an ethnologist. A problem was suggested to him by Professor R. M. Yerkes,—evidently difficult and yet extremely fascinating. Its thorough study would certainly require years of diligent work. But the writer, because of his ethnological interests, was able to give only one year to this psychological investigation.

Obviously enough, from what follows, the materials to be presented are fragmentary and inadequate for the description of the differences in the strains of rat. Still, to throw them away would seem too extravagant. With a humble sense of obligation, the writer offers his limited data to the scientific world. He wishes to take this opportunity to thank Professor Yerkes, Dr. R. M. Elliott, and Dr. W. R. Miles, for valuable assistance in the work.

PROBLEMS

The chief problem was to discover, if any, the temperamental differences between outbred and inbred strains of the albino rat. Such features of behavior as degree of nervousness or timidity, of savageness and wildness, of sensitiveness to stimuli, of persistence in response, quickness of response, and so on, may be recorded as constituting the temperament of an animal. In the terms of psychology, and in the last analysis, perhaps, temperament is identical with the threshold, quickness, amount, and steadiness of response to a given stimulus or object. The

problem, therefore, requires the measurement of the essential components of temperament in order that comparisons of the two strains may be made.

Although the inquiry was directed mainly to temperamental characteristics, differences in behavior of other sorts were noted and may here be reported. From the anthropologist's point of view, the study of close inbreeding and its consequences, even in case of lower animals, is of extreme interest and of some practical importance. Anthropological data concerning this matter are meager, and as Topinard remarks, "the question is still *sub judice*." Possibly it is not far from the truth to say that such information concerning man will long be lacking, whereas, through the study of infra-human organisms, we can readily approach reliable information. Infra-human psychology gains in importance as it allies itself with human psychology, and this paper, if not projected upon the background of larger human interests, will lose much of its significance.

There is such meager literature on temperamental characteristics of lower animals that a historical summary is unnecessary. The contribution of Basset¹ to the study of albino rats alone has fairly direct bearing upon the materials of this paper.

SUBJECTS

Only albino rats were observed. All were obtained either from the Wistar Institute of Anatomy and Biology in Philadelphia or from Miss A. E. C. Lathrop, Granby, Mass. The several inbred rats were from the inbred strain of the Wistar Institute. We are greatly indebted for them to Dr. H. H. Donaldson and Dr. H. D. King. The accompanying list supplies the reader with all available data concerning individuals on whom the observations of this report were made.

OUTBRED STOCK

Number of rat	Source and parentage.	Date of birth	Experiment begun
251 ♂	Granby*	August 5, 1914	October 14, 1914
252 ♀	"	"	"
253 ♂	"	"	"
254 ♀	"	"	"

¹ Basset, G. C. Habit formation in a strain of albino rats of less than normal brain weight. *Behavior Monographs*, 1914, 2, no. 4.

OUTBRED STOCK—*Continued*

Number of rat	Source and parentage	Date of birth	Experiment begun
1 ♂	Wistar.....	September —, 1914...	November 5, 1914
2 ♀	"	"	"
3 ♂	"	"	"
4 ♀	"	"	"
261 ♂	Granby, 251 ♂ x 252 ♀ ..	October 28, 1914.	March 2, 1915
262 ♀	"	"	"
263 ♂	"	November 28, 1914. ..	"
264 ♀	Wistar, 3 ♂ x 4 ♀	November 25, 1914. ..	"
265 ♂	"	"	"
266 ♀	Granby, 251 ♂ x 252 ♀ ..	November 28, 1914. ..	"

* Purchased of Abbie E. C. Lathrop, Granby, Mass.

INBRED STOCK

Number of rat	Source and generation of inbreeding	Date of birth	Experiment begun
201 ♂	Wistar, 14th.....	August 8, 1914.....	October 14, 1914
202 ♀	"	"	"
203 ♂	"	"	"
204 ♀	"	"	"
5 ♂	"	September —, 1914.....	November 5, 1914
6 ♀	"	"	"
7 ♂	"	"	"
8 ♀	"	"	"
211 ♂*	Wistar, 15th.....	December 18, 1914.....	April 11, 1915
212 ♀*	"	"	"
213 ♂*	"	"	"
214 ♀*	"	"	"
215 ♂*	"	"	"
216 ♀*	"	"	"

* Offspring of 201 ♂ x 202 ♀.

METHOD OF INQUIRY

Observations were made for above strains of rat as nearly as possible at the same age, and for the sake of comparability, on the same day. Certain of the observations were made under the natural cage conditions; others, under definite experimental conditions and for very specific purposes. These two kinds of data, contrasted as the naturalistic and the experimental, tend to supplement one another.

The naturalistic type of observation includes (1) observation

of the position of an individual in the cage or nest box; (2) of the relative positions of the two individuals, male and female, in the cage; (3) of the degree of activity in the cage; (4) of savageness, viciousness, or tendency to bite. The experimental observation includes (1) measurements of quickness of response to auditory stimuli; (2) of amount of movement in response to the same stimuli; (3) of general behavior (restlessness) during stimulation.

These several varieties of observation will now be reported in tabular form, with scant discussion.

POSITION OF RAT IN CAGE

The animals were kept in rectangular, all-wire cages, the floor dimensions of which were 16 inches by 14 inches. A single pair of individuals, either outbred or inbred, was kept in a cage. When the writer came into the animal room to feed the rats, many of them would, as a rule, come forward in expectation of food, but some would remain at the back of the cage or retreat to the distant portion of the cage as the experimenter approached. Some came forward singly; others, together. Sometimes the individuals were found lying together in the cage; at other times they were observed to be distant from one another. The data of table 1 concern, first, position of the two animals, male and female, in the cage when the experimenter entered the room; and second, the positions of male and female with relation to one another.

TABLE 1

POSITION OF RATS IN CAGE

OUTBRED RATS

Date	Number of rat	Position forward	Male and female together
Oct. 15 on.....	251 ♂	$\frac{77}{110} = .70$	$\frac{9}{110} = .08$
"	253 "	$\frac{76}{100} = .76$	$\frac{7}{100} = .07$
Mar. 2 on.....	261 "	$\frac{40}{60} = .67$	$\frac{12}{60} = .20$
"	263 "	$\frac{41}{60} = .68$	$\frac{18}{60} = .30$

TABLE 1—*Continued*

Date	Number of rat	Position forward	Male and female together
"	265 ♂	$\frac{57}{60} = .95$	$\frac{27}{60} = .45$
Nov. 5 on	1 "	$\frac{65}{90} = .72$	$\frac{9}{90} = .10$
"	3 "	$\frac{43}{80} = .54$	$\frac{23}{80} = .29$
Average		$\frac{437}{700} = .62$	$\frac{98}{700} = .14$
Oct. 15 on	252 ♀	$\frac{82}{110} = .75$	$\frac{9}{110} = .08$
"	254 "	$\frac{71}{100} = .71$	$\frac{7}{100} = .07$
Mar. 2 on	262 "	$\frac{47}{60} = .78$	$\frac{12}{60} = .20$
"	264 "	$\frac{10}{60} = .17$	$\frac{18}{60} = .30$
"	266 "	$\frac{57}{60} = .95$	$\frac{27}{60} = .45$
Nov. 5 on	2 "	$\frac{76}{90} = .84$	$\frac{9}{90} = .10$
"	4 "	$\frac{65}{90} = .72$	$\frac{23}{80} = .29$
Average		$\frac{490}{700} = .70$	$\frac{98}{700} = .14$

POSITION OF RATS IN CAGE

INBRED RATS

Date	Number of rat	Position forward	Male and female together
Oct. 15 on	201 ♂	$\frac{40}{110} = .36$	$\frac{43}{110} = .39$
"	203 "	$\frac{40}{120} = .33$	$\frac{24}{50} = .48$
Mar. 2 on	211 "	$\frac{26}{30} = .87$	$\frac{11}{30} = .37$

TABLE 1—*Continued*
 INBRED RATS—*Continued*

Date	Number of rat	Position forward	Male and female together
Mar. 2 on.....	213 ♂	$\frac{30}{30} = 1.00$	$\frac{19}{30} = .63$
"	215 "	$\frac{27}{30} = .90$	$\frac{12}{30} = .40$
Nov. 5 on.....	5 "	$\frac{28}{80} = .35$	$\frac{41}{80} = .51$
"	7 "	$\frac{63}{90} = .70$	$\frac{22}{90} = .24$
Average.....		$\frac{306}{700} = .44$	$\frac{293}{700} = .42$
Oct. 15 on.....	202 ♀	$\frac{39}{110} = .35$	$\frac{43}{110} = .39$
"	204 "	$\frac{26}{50} = .52$	$\frac{24}{50} = .48$
Mar. 2 on.....	212 "	$\frac{26}{30} = .87$	$\frac{11}{30} = .37$
"	214 "	$\frac{29}{30} = .97$	$\frac{19}{30} = .63$
"	216 "	$\frac{27}{30} = .90$	$\frac{12}{30} = .40$
Nov. 5 on.....	6 "	$\frac{31}{90} = .34$	$\frac{49}{90} = .54$
"	8 "	$\frac{58}{90} = .64$	$\frac{22}{90} = .24$
Average.....		$\frac{329}{700} = .47$	$\frac{293}{700} = .42$

The fractions of column 3 in this table indicate the relative frequency of the forward position, that is, the presence of a rat forward or its movement to that position on the approach of the experimenter. The numerator of the fraction indicates frequency of this position; the denominator indicates the total number of observations on the individual.

The chief results of these observations on the position of

the animals in the cage and their relation to one another may be stated thus:

(1) Outbred individuals more frequently come forward in the cage than inbred.

(2) Females more frequently come forward than males.

(3) Individual differences are greater than are the differences between the two strains or the two sexes.

(4) Inbred males and females are found together 3 times as frequently as are outbred males and females.

ACTIVITY

The degree of activity of the several rats, as indicated by their walking, running, climbing, washing, sniffing, seeking for food, lifting the lid of the cage, and so on, was observed and roughly graded by means of the numerals 0 to 5, 0 indicating minimum activity and 5 maximum activity.

As in the previous case, tabular presentation is possible, and in table 2 appear the comparable data for the two strains. In this table, the numerator of the fraction is the sum of the various grades given an animal in the total number of observations, which appears as the denominator of the fraction. The average grade in decimals in each case follows the fraction.

TABLE 2
AMOUNT OF ACTIVITY OF RATS

Outbred			Inbred		
Date	No. of rat	Activity	Date	No. of rat	Activity
Oct. 15 on....	251 ♂	286	Oct. 15 on....	201 ♂	163
		110			110
		221			103
"	253 "	100	"	203 "	120
		145			66
		60			30
Mar. 2 on....	261 "	103	Mar. 2 on....	211 "	59
		60			30
		103			30
"	263 "	60	"	213 "	70
		94			30
		60			30
"	265 "	60	"	215 "	70
		60			30
		60			30

TABLE 2—*Continued*

Outbred			Inbred		
Date	No. of rat	Activity	Date	No. of rat	Activity
		191			116
Nov. 5 on....	1 ♂	$\frac{90}{191} = 2.1$	Nov. 5 on....	5 ♂	$\frac{80}{116} = 1.5$
"	3 "	$\frac{101}{80} = 1.3$	"	7 "	$\frac{174}{90} = 1.9$
Average.....		$\frac{139}{70} = 2.0$	Average.....		$\frac{123}{70} = 1.8$
Maximum.....		2.6	Maximum.....		2.3
Minimum.....		1.3	Minimum.....		.9
Oct. 15 on....	252 ♀	$\frac{266}{110} = 2.4$	Oct. 15 on....	202 ♀	$\frac{174}{110} = 1.6$
"	254 "	$\frac{248}{100} = 2.5$	"	204 "	$\frac{51}{50} = 1.0$
Mar. 2 on....	262 "	$\frac{147}{60} = 2.5$	Mar. 2 on....	212 "	$\frac{68}{30} = 2.3$
"	264 "	$\frac{67}{60} = 1.1$	"	214 "	$\frac{54}{30} = 1.8$
"	266 "	$\frac{130}{60} = 2.2$	"	216 "	$\frac{62}{30} = 2.1$
Nov. 5 on....	2 "	$\frac{213}{90} = 2.4$	Nov. 5 on....	6 "	$\frac{120}{90} = 1.3$
"	4 "	$\frac{202}{90} = 2.2$	"	8 "	$\frac{143}{90} = 1.6$
Average.....		$\frac{152}{70} = 2.2$	Average.....		$\frac{117}{70} = 1.7$
Maximum.....		2.5	Maximum.....		2.3
Minimum.....		1.1	Minimum.....		1.0

Three comparative statements are justified by the data of table 2:

- (1) Outbred rats are more active than inbred.
- (2) The activity of the sexes differs much more for the outbred strain than for the inbred.
- (3) Individual differences in activity are greater than either sex or strain differences.

SAVAGENESS OR VICIOUSNESS AS INDICATED BY THE
TENDENCY TO BITE

As a means of testing the savageness of the rats, a copper wire was thrust into the cage from the front and from above, and the floor was scraped or scratched with it. Some individuals would, at this, dash forward and bite viciously and persistently at the wire, whereas others merely noticed the disturbance and were otherwise indifferent to it. The method of grading this behavior was similar to that used in the case of activity.

The essential statistical data from these observations appear as table 3.

TABLE 3

SAVAGENESS OR VICIOUSNESS (BITING) OF RATS

Outbred			Inbred		
Date	No. of rat	Savageness (biting)	Date	No. of rat	Savageness (biting)
Oct. 15 on....	251 ♂	$\frac{92}{110} = .84$	Oct. 15 on....	201 ♂	$\frac{31}{110} = .28$
"	253 "	$\frac{4}{100} = .04$	"	203 "	$\frac{109}{120} = .91$
Mar. 2 on....	261 "	$\frac{2}{60} = .03$	Mar. 2 on....	211 "	$\frac{79}{30} = 2.63$
"	263 "	$\frac{1}{60} = .02$	"	213 "	$\frac{74}{30} = 2.47$
"	265 "	$\frac{48}{60} = .80$	"	215 "	$\frac{26}{30} = .87$
Nov. 5 on....	1 "	$\frac{10}{90} = .11$	Nov. 5 on....	5 "	$\frac{6}{80} = .08$
"	3 "	$\frac{34}{80} = .43$	"	7 "	$\frac{35}{90} = .39$
Average.....		$\frac{226}{700} = .32$	Average.....		$\frac{763}{700} = 1.09$
Maximum.....		.84	Maximum.....		2.63
Minimum.....		.02	Minimum.....		.08
Oct. 15 on....	252 ♀	$\frac{6}{110} = .05$	Oct. 15 on....	202 ♀	$\frac{86}{110} = .78$
"	254 "	$\frac{52}{100} = .52$	"	204 "	$\frac{161}{50} = 3.22$

TABLE 3—*Continued*

Outbred			Inbred		
Date	No. of rat	Savageness (biting)	Date	No. of rat	Savageness (biting)
Mar. 2 on....	262 ♀	$\frac{224}{60} = 3.73$	Mar. 2 on....	212 ♀	$\frac{83}{30} = 2.77$
"264 "	$\frac{99}{60} = 1.65$	"214 "	$\frac{22}{30} = .73$
"266 "	$\frac{60}{60} = 1.00$	"216 "	$\frac{87}{30} = 2.90$
Nov. 5 on....	2 "	$\frac{209}{90} = 2.32$	Nov. 5 on....	6 "	$\frac{163}{90} = 1.81$
"4 "	$\frac{301}{90} = 3.34$	"8 "	$\frac{440}{90} = 4.89$
Average.....		$\frac{1261}{700} = 1.80$	Average.....		$\frac{1710}{700} = 2.44$
Maximum.....		3.73	Maximum.....		4.89
Minimum.....		.05	Minimum.....		.73

The females far exceeded the males in degree of savageness. They were, moreover, surprisingly quick and aggressive, whereas the males were either on the defensive or indifferent. Whether this remarkable sex difference is correlated with the maternal instinct is not clear. It is, however, noteworthy that in the feeding behavior the male very obviously asserts himself and becomes the aggressor. It further appears from these observations that the tendency to bite is not directly proportional to activity. Instead, there seems to be a tendency toward an inverse relation.

The chief facts concerning savageness in the two strains are these:

- (1) Females are much more savage (exhibit the tendency to bite more often and persistently) than are males.
- (2) The inbred rats are much more savage than the outbred.
- (3) Individual differences exceed either sex or strain differences, and they are especially great in case of the females.

EXPERIMENTAL OBSERVATIONS

The writer had intended to record the responses of his animals to various stimuli and to more complex situations by means of the galvanometer. But as the resources of the laboratory at

the time did not permit of the development of a suitable form of apparatus, this plan was abandoned in favor of a less expensive and cruder preliminary mode of observation.

The apparatus finally devised and used consisted of a small blackened rectangular box which rested at one end on two pointed metallic posts and was suspended at the other end by a delicate spring. This box was connected with a kymograph by means of a marking lever so that any vertical movement of the box was recorded on the kymograph surface. The rat was placed in the box and so confined by means of movable partitions that it was forced to hold its orientation with head pointed forward toward the writing lever. The front end of the box consisted of a wire screen. On the kymograph three records were written: (1) A time line, indicating fifths of a second; (2) a stimulus line; (3) a response line.

The only mode of stimulation here reported is the auditory, and for this purpose an electric bell was used.

The reaction box and stimulus apparatus were enclosed in a large pasteboard box in order that the animal should be somewhat protected from disturbing conditions.

QUICKNESS OF RESPONSE TO AUDITORY STIMULI

A rat having been placed in the apparatus and allowed to become accustomed to its position, the various parts of the mechanism were carefully adjusted, and when everything was in readiness an auditory stimulus was given for .5 to .6 of a second. After eight seconds, the stimulus was repeated. Then the experimenter waited for an interval of another eight seconds before again presenting a pair of auditory stimuli.

The quickness of response was indicated by the distance between the point of stimulation on the stimulus line and the point of initial response on the reaction line. The measurement is extremely crude and inaccurate, but so far as may be judged, not more so for the one strain or the one sex than for the other. Where, because of long delayed or indefinite response, it was difficult to decide on the initial point, the reaction was ignored.

The data of table 4 include the mean or average reaction time for each individual in the first trial, that is, after initial stimulus, and in the second trial, that is, with repetition of the stimulus, the maximal and minimal reaction times, the total

number of trials, and the number of trials in which no response appeared. The chief results of these measurements of reaction time may be stated thus:

TABLE 4
QUICKNESS OF RESPONSE TO AUDITORY STIMULUS
OUTBRED

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
251 ♂	.35"*	.8"*	0*	3	10	.98"*	1.8"*	0	5	10
261 "	.39"	.9"	0	8	17	.21"	.35"	0	12	17
263 "	.51"	1"	0	3	16	.30"	1.2"	0	5	16
265 "	.51"	1"	0	7	15	.46"	1.2"	0	9	15
	Aver. .47"	1"	0	Total 21	Total 58	Aver. .32"	1.2"	0	Total 31	Total 58

Average reaction time for the first and second trials..... = .40"

Total number of failures to respond in the first and second trials = 52

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
252 ♀	.43"*	1.0"*	0	3	10	.63"*	1.8"*	0	1	10
262 "	.38"	.7"	0	4	17	.43"	.95"	0	7	17
264 "	.15"	.3"	0	5	17	.30"	.7"	0	4	17
266 "	.28"	.6"	0	6	14	.80"	.8"	0	9	14
	Aver. .27"	.7"	0	Total 18	Total 58	Aver. .51"	.95"	0	Total 21	Total 58

Average reaction time for the first and second trials..... = .39"

Total number of failures to respond in the first and second trials = 39

* Excluded from the averages, as the accuracy of measurements was uncertain.

INBRED

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
201 ♂	.42"*	.7"*	.08"*	0	10	.33"*	.7"*	0*	1	10
211 "	.18"	.3"	0	3	16	.26"	1.2"	0	3	16
213 "	.19"	.3"	0	1	16	.14"	.3"	0	9	16
215 "	.17"	.4"	0	2	16	.13"	.2"	0	7	16
	Aver. .18"	.4"	0	Total 6	Total 58	Aver. .18"	1.2"	0	Total 20	Total 58

Average reaction time for the first and second trials..... = .18"

Total number of failures to respond in the first and second trials = 26

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
202 ♀	.26"*	2.0"*	.01"*	1	10	.30"*	.6"*	0	3	10
212 "	.23"	.85"	0	3	16	.22"	.65"	0	4	16
214 "	.29"	.5"	0	8	16	.08"	.08"	0	13	16
216 "	.28"	.8"	0	6	16	.23"	.8"	0	5	16
	Aver. .27"	.85"	0	Total 18	Total 58	Aver. .18"	.8"	0	Total 25	Total 58

Average reaction time for the first and second trials..... = .23"

Total number of failures to respond in the first and second trials = 43

* Excluded from the averages as the accuracy of measurements was uncertain.

(1) Inbred rats respond more quickly to the auditory stimulation than do outbred. (a) Inbred males respond most quickly of all (.18 seconds). (b) Inbred females rank next in quickness of response (.23 seconds). (c) Outbred females rank third (.39 seconds). (d) Outbred males are slowest of all (.40 seconds).

(2) The number of failures to respond obviously to the auditory stimulation is both smallest and greatest for the males. (a) The inbred males failed to respond 26 times in 116 trials. (b) The outbred females failed to respond 39 times in 116 trials.

- (c) The inbred females failed to respond 43 times in 116 trials.
 (d) The outbred males failed to respond 52 times in 116 trials.

(3) The reaction time of the males is extremely variable; that of the females is comparatively uniform.

(4) The sex difference in reaction time for inbred rats is slightly more than for outbred.

(5) Reaction time, with certain exceptions, is shorter in case of the second trial (repetition of auditory stimulus) than in the first. Failures to respond are more frequent in the second trial than in the first.

(6) Individual differences exceed sex and strain differences.

AMOUNT OF RESPONSE TO AUDITORY STIMULI

The amount of motor response to auditory stimuli was determined by multiplying the maximum length (straight line) of the response curve by the maximum height (straight line). The result is expressed in square centimeters. These measurements are even less reliable than those of reaction time, but again there seems no reason to suppose that the errors are unequal for either the sexes or the strains of rat.

As previously, the statistical data are arranged in tabular form. They appear in table 5.

TABLE 5
 AMOUNT OF RESPONSE TO AUDITORY STIMULI
 OUTBRED

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
251 ♂	16.3	64	0	3	10	15.2	32	0	5	10
261 "	3.1	20	0	10	17	1.1	10	0	14	17
263 "	8.9	40	0	6	16	2.6	40	0	10	16
265 "	6.0	20	0	8	14	6.4	40	0	9	14
	Aver.			Total	Total	Aver.			Total	Total
	8.6	64	0	27	57	6.3	40	0	38	57

Average amount of response for the first and second trials. . . . = 7.5 c.c.

Total number of failures to respond in the first and second trials = 65

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
252 ♀	65.6	280	0	3	10	39.7	168	0	1	10
262 "	24.0	120	0	5	17	11.5	88	0	9	17
264 "	7.9	48	0	5	16	6.0	20	0	4	16
266 "	15.4	48	0	5	13	1.4	14	0	9	13
	Aver. 28.2	280	0	Total 18	Total 56	Aver. 14.7	168	0	Total 23	Total 56

Average amount of response for the first and second trials. . . . = 21.5 c.c.

Total number of failures to respond in the first and second trials = 41

INBRED

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
201 ♂	18.1	36	2	0	16	11.3	72	0	1	16
211 "	13.9	50	0	3	16	2.9	12	0	5	16
213 "	24.1	120	0	4	16	7.0	40	0	10	16
215 "	27.9	180	0	3	16	8.2	80	0	8	16
	Aver. 21.0	180	0	Total 10	Total 64	Aver. 7.4	80	0	Total 24	Total 64

Average amount of response for the first and second trials. . . . = 14.2 c.c.

Total number of failures to respond in the first and second trials = 34

Number of rat	First trial					Second trial				
	Mean	Max.	Min.	No resp.	No. of trials	Mean	Max.	Min.	No resp.	No. of trials
202 ♀	133.1	640	0	1	16	35.3	192	0	3	16
212 "	29.0	96	0	4	16	17.1	168	0	7	16
214 "	14.4	48	0	8	16	11.3	144	0	12	16
216 "	9.5	44	0	6	16	8.0	48	0	10	16
	Aver. 46.5	640	0	Total 19	Total 64	Aver. 17.9	192	0	Total 32	Total 64

Average amount of response for the first and second trials. . . . = 32.2

Total number of failures to respond in the first and second trials = 51

The following conclusions are indicated by the measurements of amount of response:

- (1) The response of outbred rats is far less than that of inbred.
- (2) The response of females is greater than that of males.
- (3) The response is less in the second trial than in the first.
- (4) Individual and sex differences in amount of response far exceed strain differences.

GENERAL BEHAVIOR DURING AUDITORY STIMULATION

As an indication of the restlessness or general tendency to activity as a result of auditory stimulation, the total length of the response curve was measured by means of a chartometer. This could be done only very inaccurately because of the many slight changes in direction of the curve, but as in the case of previous experiments, there is no reason to suppose that the degree of accuracy varies with the strains or with the sexes.

Measurements of restlessness were made for two conditions of stimulation: (a) For separate auditory stimuli, the one following the other after a stated interval and thus giving in the records trial 1 and trial 2; and (b) for continuous auditory stimulation instead of momentary.

Table 6 presents the data of restlessness or general behavior.

The principal results may be summarily stated thus:

TABLE 6
BEHAVIOR DURING EXPERIMENT
OUTBRED

Number of rat	With intermittent stimulation				With continuous stimulation			
	Mean	Max.	Min.	No. of trials	Mean	Max.	Min.	No. of trials
251 ♂	17.4*	19.0	17.0	10	18.1	20.0	17.1	3
261 "	17.2	17.5	17.0	18	17.2	18.5	17.0	18
263 "	17.3	19.0	17.0	17	17.2	17.8	17.0	17
265 "	17.6	21.8	17.0	15	17.3	18.0	17.0	15
	Average 17.4	21.8	17.0	Total 60	Average 17.5	20.0	17.0	Total 53

Average of both responses to intermittent and continuous stimulations = 17.5

Number of rat	With intermittent stimulation				With continuous stimulation			
	Mean	Max.	Min.	No. of trials	Mean	Max.	Min.	No. of trials
252 ♀	22.5	38.2	17.0	10	23.3	26.2	20.5	2
262 "	17.4	18.0	17.0	18	19.5	38.5	17.0	18
264 "	18.4	22.7	17.0	17	18.4	21.0	17.0	17
266 "	17.3	18.5	17.0	14	18.1	21.0	17.0	14
	Average 18.9	38.2	17.0	Total 59	Average 19.8	38.5	17.0	Total 51

Average of both responses to intermittent and continuous stimulations = 19.4

* Expressed in centimeters.

INBRED

Number of rat	With intermittent stimulation				With continuous stimulation			
	Mean	Max.	Min.	No. of trials	Mean	Max.	Min.	No. of trials
201 ♂	17.7	20.0	17.0	10	17.5	18.0	17.0	5
211 "	17.8	24.0	17.0	16	18.9	21.6	17.1	16
213 "	17.2	17.5	17.0	16	17.6	21.0	17.0	16
215 "	19.2	18.2	17.0	16	17.4	20.0	17.0	16
	Average 18.0	24.0	17.0	Total 58	Average 17.9	21.6	17.0	Total 53

Average of both responses to intermittent and continuous stimulations = 18.0

Number of rat	With intermittent stimulation				With continuous stimulation			
	Mean	Max.	Min.	No. of trials	Mean	Max.	Min.	No. of trials
202 ♀	20.1	30.5	17.0	11	17.3	17.5	17.1	5
212 "	17.7	23.0	17.0	16	17.8	22.0	17.0	16
214 "	18.2	21.5	17.0	16	18.7	26.0	17.0	16
216 "	17.6	24.5	17.0	16	17.6	24.0	17.0	16
	Average 18.4	30.5	17.0	Total 59	Average 17.9	26.0	17.0	Total 53

Average of both responses to intermittent and continuous stimulations = 18.2

(1) For inbred rats, the restlessness or continuity of response is greater in case of momentary and repeated auditory stimulation, whereas for outbred rats, the reverse is true.

(2) The amount of restlessness varies more widely for outbred than for inbred rats.

(3) The females of both strains show higher records for restlessness than do the males.

(4) The records for the females are also more variable than for the males, with averages as follows: Outbred males, 17.5;

inbred males, average 18.0; outbred females, average 19.4; inbred females, average 18.2.

(5) Individual and sex differences greatly exceed strain differences.

CONCLUSIONS

The outbred and inbred strains of albino rats contrast in temperament as follows:

(1) Inbred males and females are found together in the cage about three times as frequently as are outbred.

(2) The inbred rats come forward in the cage on the approach of the experimenter much less frequently than do the outbred.

(3) Inbred rats are less active than outbred.

(4) The inbred stock exhibits savageness by biting to approximately twice the extent of the outbred.

(5) The inbred animals respond more quickly and in greater amount to momentary auditory stimulation than the outbred.

(6) The two strains differ also in restlessness or continuity of response. For the inbred restlessness is greatest in case of momentary and repeated auditory stimulation and less in case of continuous stimulation, whereas for the outbred animals, the reverse is true.

(7) The data indicate less difference between the sexes in the inbred than the outbred rats.

RETROACTIVE ASSOCIATION AND THE ELIMINATION OF ERRORS IN THE MAZE

HELEN B. HUBBERT AND K. S. LASHLEY

The temporal relation of the different activities which are associated in the formation of a habit seems to have a direct bearing upon the form in which the habit is fixed. Thus in an experiment described by Bohn (Dontchef-Dezeuze, '14), carried out in Pawlow's laboratory, the selection of stimulus and response by their temporal relation seems established. In this experiment an electrical stimulus was applied to the skin of a dog, eliciting struggling and howling. Following this, food was placed in the animal's mouth and salivary secretion was obtained as a result. After this sequence had been repeated many times the only reaction to the electrical stimulation of the skin was the secretion of saliva. Why, in this experiment, was the secretion of the saliva associated with the electrical stimulus and not the struggling and howling with the taste of food? An animal may be trained readily to reject food with the latter reactions in a given situation by the use of punishment after food is taken,¹ so it seems that the temporal order determined which of the stimuli and responses were to become associated.

In the formation of complex habits it is probable that similar and even more complex temporal factors modify the course of learning. Hachet-Souplet ('13) has stated dogmatically that when a series of actions, leading up to a pleasurable situation, becomes connected into a habit the association occurs first between the activities just preceding the pleasant result and progresses to those more remotely antecedent. This may be called the principle of retroactive association. If it is a fact it has an important bearing upon theories of the mechanism of selection in habit-formation.

In the work with the conditioned reflex it has been shown that a well established reflex may serve as a basis for the formation

¹ Hachet-Souplet ('14) describes an interesting case of this sort.

of other conditioned reflexes as well as can an unconditioned reflex (Bechterew, '13). If this condition obtains in the formation of a maze-habit, the situation involved in the last turn before the food is reached may acquire from the getting of food something (we can not be more definite at present) which makes it capable of serving instead of the food in the fixation of the next preceding activity. In this case the formation of a maze-habit would appear as a series of secondary, tertiary, etc., conditioned reflexes starting from the taking of food as the primary reflex and progressing from the food compartment outward to the entrance of the maze. Such an explanation avoids one of the chief difficulties of theories of the fixation of habit; that of accounting for the effects of getting food, or what not, upon an activity which occurred half an hour or more earlier, such as turning in a given direction at the entrance to the maze.²

But such an hypothesis must be looked upon with suspicion until the fact of retroactive association is established, and there is little evidence in favor of this at present. With a slightly different problem in view Hubbert ('15) has made an analysis of the order of elimination of errors in the maze. The results of this analysis were not very certain owing to confliction in the data given by different groups of animals. One point was quite clear, however; there is no invariable sequence in the elimination of errors, if the records of single animals are considered. In contrast to this, when the averages of very large groups of animals are taken there does seem to be a progressive elimination of errors from the food compartment to the entrance of the maze. This is shown by the following averages based upon all the data given by Hubbert. (See figure 1 for the designation of the alleys.)

a	b	c	d	e	f	
30.6	26.4	19.7	19.7	18.7	8.3	trials.

² The theories of nervous drainage such as those formulated by Pawlow, Max Meyer, Watson, and others seem to require the immediate succession of the acts associated. In the experiments of Bohn, for example, they must assume that the salivary reflex is excited while the nervous elements of the struggling reflexes are still active and that the efferent fibres of the salivary glands drain off a part of the energy from the cutaneous stimulation, thus opening the synapses from the receptors of the skin to the salivary glands. None of the other theories of the physiology of learning has been formulated in sufficient detail to take into consideration the possibility of retroactive association.

In the individual records we can not be certain either that a single correct turn made by an animal in the maze proves that the animal has learned the turn, or that a single error proves the absence of the habit for the particular turn involved. The former may be due to chance, the latter to a distracting stimulus whose threshold of reaction is lower than that of the maze-habit. In a question of this sort the most dependable results are therefore to be obtained from averages, which rule out, to a certain extent at least, the chance successes and errors. It may be that while individual records do not show an exact progressive elimination of errors, the larger averages do reveal this



FIGURE 1.—Ground plan of circular maze, showing the position of the errors studied.

as one of a number of factors influencing the course of learning. There is some doubt as to the comparability of the different alleys in the circular maze and no great significance can be attached to the progression except in the case of alleys b, c, d and e, which are strictly comparable.

Vincent ('15, IV) has given data which indicates a more pronounced progressive elimination than appears in the work of Hubbert. In the form in which this is presented, however, Vincent is not justified in applying the data to the problem. She makes no statement as to what constituted a trial in her experiments and in the two "typical records" of trials which she reports in detail ('15, I) we find that the rat, after reaching the food, was allowed to return and re-explore the maze. In the first trial of her rat "No. 1" the animal was allowed to leave

the food compartment and explore the inner *cul de sacs* twice before the trial was ended, and in the second trial the animal left the food and explored the inner alleys five times. If the final data is based upon trials conducted with this technique it can have no bearing upon the problem of the elimination of errors, for we can not determine whether the smaller number of trials required for the elimination of the errors near the food box resulted from retroactive association or from the fact that additional trials in the inner part of the maze were ignored.

Additional evidence dealing with the question of retroactive association is now at hand and makes possible a more positive conclusion concerning its rôle in habit-formation than could be drawn before. The evidence consists of data upon the elimination of errors during the training of 56 rats in the Watson circular maze. The method of analysis differs from that employed earlier by Hubbert in that different types of errors are treated separately.

The ground-plan of the maze is given in figure 1. As will be noted, two chief types of error are distinguishable. The first of these (type I) is the passing of a doorway through which the animal should go. Errors of this type are marked with even numbers in the figure. The other (type II) is that of an incorrect turn after passing through the doorway. Errors of this type are given odd numbers in the figure. In addition to these types are errors of turning back upon the correct pathway. While common enough in the early part of training these errors are soon eliminated. In the 616 possible cases in the data examined only one was found in which an error of turning back followed the last error of types I and II in any alley (with the exception of *f*, which, being without a partition, permits only errors of turning back), so that they may be disregarded in any study which deals only with the last error made at each given point in the maze. In compiling the data any turn which carried the animal for its own length or more into a blind alley was counted as an error. Thus figure 2, a tracing of the path followed in a single trial, shows errors at 2, 5, 7, and 11. The records of all the animals were examined and note made of the number of trials preceding the last in which each of the 11 possible errors was made. The averages of these for the 56 rats are given in table 1.

TABLE 1

THE AVERAGE NUMBER OF TRIALS REQUIRED BY THE 56 RATS FOR THE ELIMINATION OF EACH OF THE 11 ERRORS POSSIBLE IN THE WATSON CIRCULAR MAZE. THE NUMERALS INDICATE THE POSITION OF THE ERRORS IN FIGURE 1.

Errors of passing a door		Errors of turning wrongly	
Number	Average trials before elimination	Number	Average trials before elimination
2	30.07	1	36.62
4	23.19	3	36.48
6	19.18	5	37.97
8	12.48	7	39.21
10	21.05	9	32.46
Averages	21.19	Averages	36.55
Number 11	30.27		

TABLE 2

THE NUMBER OF CASES IN WHICH EACH OF THE BLIND ALLEYS WAS NOT EXPLORED ONCE BEFORE THE MAZE-HABIT WAS THOROUGHLY ESTABLISHED

Errors of passing a door		Errors of turning wrongly	
Number	Number of cases	Number	Number of cases
2	0	1	0
4	2	3	0
6	3	5	0
8	22	7	0
10	4	9	2
Total	31	Total	2

The most striking fact brought out by this table is that errors of type I are eliminated in less than two-thirds as many trials as are those of type II. Many rats never made certain errors and the distinction between the types is shown here also. Table 2 gives the number of cases in which each of the errors was not made once during the course of training. Blind alleys, entrance to which constitutes an error of type II remained unexplored in only two cases. In contrast to this, blind alleys beyond the doorways remained unexplored in 31 cases.

What is the explanation of this fact? The first suggestion is that there is a transfer of the reaction learned for one doorway to other situations, but it may be also that a break in the smooth walls of the alleys excites some instinctive mechanism which carries the animals through the doorways. To test this latter possibility the records were examined to discover whether the animals more frequently went through the first door that they came to in their first trial in the maze or passed beyond it into the blind alley, that is, whether or not error 2 was made at the first possible opportunity. It was found that, of the 56 records

of first trials 32 showed that the animal turned through the first doorway and 12 that he passed the doorway. The remaining 12 were uncertain owing to the confusion of lines made in following the movements of the rats. Thus in three-fourths of the available cases the animals, without previous training, turned through the doorway instead of passing it; a fact which furnishes evidence for an instinctive basis for the elimination of errors of type I. It is probable that this accounts for the difference in time required for the elimination of the two types of errors.



FIGURE 2.—Tracing of path followed in one trial, showing method of counting errors. Errors were made in this trial at 2, 5, 7 and 11.

An additional difference between the types of errors, which is not explained by instinctive behavior, is the order of elimination within the series. Among the errors of type I there is a very marked reduction in the number of trials required for the elimination of successive errors from the circumference to the center of the maze. Nothing of the sort is apparent in errors of type II.³ Is there a retroactive association in the case of

³ There is good reason for disregarding the two errors which are not in conformity with the others, errors 10 and 11. After reaching the food at the center of the maze the rats frequently turn back and explore the inner alleys of the maze before eating. These exploratory activities constitute one of the most characteristic instinctive activities of the rat. In the home cages they are usually not evident in the scramble for food which takes place when several rats are together in a familiar place, but in all relatively new situations the animals rarely begin to eat until they have made a thorough exploration of their surroundings. In the maze the field of exploration usually includes the food compartment, the alley *f*, and its doorway which leads to error 10. When the rats have learned the maze they may eat as soon as they reach the food, but any strange odor or startling noise may lead to a re-

errors of type I and not of type II? If such association, in the sense of a serial formation of conditioned reflexes, is an important principle in habit-formation it must be obscured by other agents in the elimination of errors of the second type. If not, it is for some reason simulated in the elimination of errors of type I. An inquiry into the relative complexity of the factors influencing the elimination of the two types of errors gives data which seems to favor the latter possibility. Movements toward the center of the maze are much more influenced by gross orientation than are those of turning to the right or left. After the third to sixth trial the rats run with their heads near the convex (inner) walls of the alleys of the maze and almost invariably confine their efforts to climb out of the alleys to these partitions. Such behavior becomes more pronounced as they approach the center of the maze and persists until the limits of training are reached. It is improbable that this is a reaction to the odor of food in the center of the maze, first, because it does not occur during the first trials and second, because smearing the maze with food does not alter the reaction essentially. It may be a reaction to the curvature of the sides of the alleys but such an association seems improbable in view of the slowness with which similar sensory habits are formed in the discrimination box. The final alternative is that of an orientation to the maze as a whole. The work of Carr shows that the animals are usually well oriented with respect to the direction of the food compartment from the starting box and may even depend upon the direction of the maze from their home cages for orientation. The behavior of the rats strongly suggests that they very soon acquire this sense of direction in the maze and that as they approach the center of the maze the orientation becomes more certain, perhaps by the summation of familiar stimuli, perhaps by the closer approximation of the visible objects above the maze to their appearance from the food-box.

The conditioning stimuli to an orientation determined either by the curvature of the alleys of the maze or by the visible tracing of this part of the path. This is evidence for an additional interfering agent in the elimination of errors 10 and 11 which does not act in the case of the other errors and justifies the view that the elimination of these errors is not directly comparable with that of the others. In the experiments reported here every effort was made to prevent this retracing of the path. The moment the animal entered the food-compartment the experimenter hastened to close the doorway between alleys *e* and *f*, and in very few cases did the rats escape into the outer alleys.

objects above them, would obviously remain nearly constant, no matter in what alley the animal happened to be. A similar orientation with respect to right and left turns could not be maintained equally well because the direction of the doorways alternates with successive alleys. We have not had opportunity to test the mechanism of orientation but there can be little doubt that the elimination of errors of type I is affected by it to a greater extent than is that of errors of type II. We can find, on the other hand, no evidence for any influence which might hide a retroactive association in the elimination of errors of type II. Such an influence would have to be equally strong and equally uniform in its action with retroactive association and the behavior of the rats gives no evidence of anything which might produce this result.

Whatever the explanation of the serial elimination of errors of type I, the process is evidently a complicated one and can not be advanced as proof of retroactive association. No such complication can be demonstrated in the elimination of errors of type II. They show a pronounced uniformity in the amount of practice necessary for their elimination; a greater uniformity than should be expected if chance agents obscured the effects of retroactive association, and it seems certain that their elimination presents a purer case of learning by the method of trial and error than does that of type I. The number of animals is large enough to make the averages dependable while still allowing as great a chance variation as is found between the averages in type II. Clearly the principle of retroactive association does not apply to errors of this type.

Since, as we have pointed out, the elimination of this type of error is probably not complicated by transfer of training from one part of the maze to another, by orientation toward the center of the maze, or by reactions to the curvature of the passages, etc., we may extend our conclusion and say that retrogressive association is not an effective mechanism in the selection and association of simple series of motor activities in habit-formation.⁴

Evidence for the absence of retroactive association does not, of course, throw much light upon the physiological mechanism

⁴ Here and elsewhere in this paper the word "association" is used to express merely the production of a new temporal relation of stimulus and reaction.

by which the getting of food fixes the habit. It indicates that this is not the formation of a series of conditioned reflexes such as was outlined in the first part of the paper. It suggests further that the same mechanism is involved in learning *similar* reactions in different parts of the maze and that the rate of learning is the same irrespective of the temporal relation of these to the getting of food.

LITERATURE CITED

- BECHTEREW, W. VON. Objective Psychologie oder Psychoreflexologie. Leipzig, 1913. Teubner.
- DONTCHEF-DEZEUZE, M. L'image et les reflexes conditionnels dans les travaux de Pavlov. Paris, Alcan.
- HACHET-SOUPLET, P. De l'animal a l'enfant. Paris, Alcan.
- 1913.
1914. Notes psychologique sur les chiens de guerre. *Jour. de Psych. norm. et path.*, **11**, 433-441.
- HUBBERT, H. B. Elimination of errors in the maze. *Jour. Animal Behav.*, **5**, 1915. 66-74.
- VINCENT, STELLA B. The white rat and the maze problem. I. The introduction of a visual control. *Jour. Animal Behav.*, **5**, 1-24.
1915. The white rat and the maze problem. IV. The number and distribution of errors—a comparative study. *Jour. Animal Behav.*, **5**, 367-374.

A CAUSAL FACTOR IN THE RELATION OF THE DISTRIBUTION OF PRACTICE TO THE RATE OF LEARNING

K. S. LASHLEY

The Department of Psychology of the Johns Hopkins University

The fact is well established that, within limits as yet undetermined, the rate of learning varies inversely as the concentration of the periods of practice. A number of hypotheses have been advanced to account for this but none of them has any experimental evidence in its support. One of them, offered by Book¹ to account for improvement in typewriting during periods of non-practice, assumes that during a long period of practice the learner may acquire certain habits which, persisting through the practice-period, limit his chance activities and hence his possibility of improvement by the method of trial and error. During rest-periods these habits, which are not very well established, may be lost, in which case a distributed practice would permit of a greater diversity of activity than a concentrated one. In other words, during a long period of practice the learner is apt to get into a rut and intervals of rest permit him to return to the problem with a new "set" and to attack it in a different way.

The hypothesis applies not only to the periods of non-practice dealt with by Book, but to any of the effects of the distribution of practice. If it is correct, a detailed analysis of behavior in the formation of any habit should show a greater diversity of activity between successive practice-periods than within single periods. In my experiments upon the acquisition of skill in archery I observed the persistence of bad methods of aiming through single periods of practice but, lacking time for detailed descriptive or photographic records, was unable to determine the rôle of these in modifying the effects of practice.²

¹ *The Psychology of Skill: with special reference to its acquisition in typewriting.* Missoula, University of Montana, 1908.

² *Acquisition of Skill in Archery.* *Carnegie Pub.* 211. 1915.

A simple method of recording significant motor activities for a study of this problem is offered by the graphic maze.³ With it records are obtainable of all the errors made by the animals being trained and it is easy to determine whether or not any of the errors persist through single periods of practice or from day to day. If such persisting errors are the real cause of the results obtained with different distributions of practice, one should expect to find, on the average, fewer errors common to the last trial of one day and the first trial of the succeeding day's practice than to any two successive trials made on the same day.

Comparison of the numbers of duplicate errors can not be made directly owing to variations in the number of errors made in different trials and the consequent difference in the probability of chance duplication.⁴ Thus if nine out of a possible twelve errors are made in each of two successive trials a larger percentage of identical errors is to be expected from pure chance than if only three errors are made in each trial. In making the comparison I have used the records of all the errors made by 56 rats in learning the circular maze, when given five trials daily. To avoid the influence of different numbers of errors in the comparison of the number of duplicate errors appearing in single and successive practice-periods the following method of computing the results was adopted.

The records of the last trial in each day's practice and of the first in the succeeding one, where a total of three or more errors appeared in the two trials, were compared and the number of errors in each, together with the number of duplicate and diverse errors, was tabulated. The records of the animal which furnished this data were then examined for a case of two successive trials on the same day each of which contained a number of errors equal to that in the corresponding trial of the pair made on successive days. The first pair of trials meeting these requirements was taken for comparison and its numbers of duplicate and diverse errors were arranged in a separate table. Where no pair could be found which met the requirements the successive trials in different practice-periods were discarded.

³ Yerkes and Kellogg. A graphic method of recording maze reactions. *Jour. Animal Behav.*, 1914, 4, 50-55.

Watson. A circular maze with camera lucida attachment. *Ibid.*, 56-59.

⁴ The methods of training animals and recording errors in the maze have been described so frequently that they need not be discussed here. The reader who is unfamiliar with the methods is referred to J. B. Watson, "Behavior," 1915.

One hundred seventy-five pairs of successive trials made in different periods of practice with an equal number of pairs made during a single day's practice were obtained. The method gives essentially a random sample of the two kinds of pairs of successive trials, with an equal number of errors and an equal distribution of errors between the members of the pairs of each series. The rôle of chance in the production of duplicate errors may therefore be disregarded, since it should be the same for both kinds of pairs, and the relative proportion of duplicate to distinct errors in the two kinds of pairs may be considered the result of the time intervening between the successive trials. In computing the errors wrong turns only in the maze were considered and only one error was counted at each turn where errors were made. The results of the analysis are given in the following table. The totals of all cases are given and the figures for the numbers of diverse and duplicate errors are directly comparable, being based upon the same number of errors with like distribution between the pairs of trials.

SUCCESSIVE PRACTICE-PERIODS

Total number of errors in last trials of first practice-periods.....	463
Total number of errors in first trials of succeeding practice-periods.....	524
Total number of <i>pairs</i> of identical errors.....	247
Total number of diverse errors.....	493

SINGLE PRACTICE-PERIODS

Total number of errors in first trials of pairs corresponding to those above.....	463
Total number of errors in succeeding trials of pairs corresponding to those above.....	524
Total number of <i>pairs</i> of identical errors.....	274
Total number of diverse errors.....	439

In both kinds of pairs the number of duplicate errors is slightly in excess of the number of diverse ones. The maze, as arranged, offers the possibility of 12 different errors and the average number of errors per trial was 2.82. The chances, then, that the number of duplicate errors would equal the number of diverse errors was only about one to eight. The fact that the numbers were equal when the data included so many cases seems to prove that there is some predetermining factor which causes errors once made to be repeated both in single practice periods and from one practice-period to the next.

A comparison of the pairs of trials made during the same period of practice with those made in successive periods shows a greater diversity of errors in the latter. In them there were 247 pairs of like errors and 493 diverse errors; that is, 49.05% of the errors made in successive trials were diverse. In the successive trials made on the same day there were 274 pairs of like errors and 439 diverse errors; 44.48% of the errors made in these successive trials were diverse. The trials separated by a 24 hour interval are thus seen to include 10% fewer duplicate errors than those without intervening time: a greater diversity of activity occurs where successive trials are separated by a considerable interval of time than where they follow each other immediately.

This is the result to be expected on the hypothesis considered above and it seems to place the latter on a firm basis. Two questions, however, are unanswered: first, is this the only factor involved in producing the results noted when different distributions of practice are used; second, is the hypothesis applicable to other types of activity than motor forms such as archery or typewriting. It is not possible at present to say how great would be the effect of the conflicting habits in retarding the rate of learning in any particular case. If the diversity of activity is reduced 10% by the concentration of practice from one to two trials per day, a 10% increase in the amount of practice necessary for learning is the least which could be expected, but the fact that the disadvantageous activities are repeated more frequently with increasing concentration of practice would help to fix them as habits and thus produce an even greater retardation of learning. At a rough estimate, the persistence of the same errors through prolonged periods of practice seems adequate to account for such retardation of learning as has been found to result from concentrated practice during the formation of motor habits.

If the same explanation is not applicable to language habits, its validity for motor habits is questionable. In view of our ignorance of the mechanism of association in such activities generalizations here must be made with caution. There can be little doubt, however, that false associations occur in the formation of language habits and it seems probable that these may interfere with learning in the same way that habits of making wrong turns in the maze interfere with the learning of the correct path.

THE COURTSHIP OF *PIERIS PROTODICE*

PHIL RAU

The mating habits of *Pieris protodice* have been so uniform in all of the cases which have come under my observation in various times and places that I feel that this phase of their behavior is fairly constant. The following cases are typical of the usual performance.

On June 12, at 5:10 p. m., many of these white butterflies were fluttering about one of their favorite haunts, a patch of white-flowered milkweed in a sunny, treeless pasture. A female was at rest on the upper surface of a leaf, with its wings spread flat and its abdomen turned directly upward into the air and held thus rigidly. A male was hovering above her with more than usual activity; frequently he would flutter very near to the upturned abdomen. Presently he grew more bold; he repeatedly approached and beat his wings against hers with a flapping motion, and darted toward her and touched her abdomen with his own, apparently in an attempt to mate, until the excitement grew intense after several such advances, and both arose on the wing and fluttered and danced and fussed in an almost quarrelsome manner in mid-air for a few seconds, when the female settled to rest on another leaf nearby in the very same position, and the whole performance was repeated. When the male again became too familiar the female flew away as before, the male following and the two fluttering 'round and 'round each other, high in the air. Then the coquettish female suddenly darted away and hid under a leaf near the ground, this time eluding her suitor.

Although there were hundreds of these white butterflies flying about and feeding upon the white flowers, only three pairs were in copulo. Perhaps the late hour in the day might account for the small number in mating.

On three occasions I chanced to see the beginning of this strange performance, but I was unable to ascertain at any time which sex made the first advance in this courtship. In each case the female was feeding or resting on the flowers, with closed wings in a normal, placid manner in so far as I could see, when

the male came dancing by on the wing. As he approached and fluttered above her she promptly assumed the strange position described above, the wings spread flat and directed slightly backward, and the abdomen upraised. In each case they flirted until the excitement became riotous and then they fluttered away and must have separated, for they were lost among the many others in the field. I have never seen the accomplishing of actual mating, but in the several pairs observed already in copulo they rested tail-to-tail. When disturbed they would fly thus, in pairs. One pair continued flight for two and one-half minutes without alighting. The leader (sex not ascertained) did all the flying and seemed merely to carry the rear one.

On one occasion the pair continued their coquetry for fifteen minutes, with several attempts at mating. During this period the female moved from flower to flower, not with an air of trying to escape, but rather leading him on; the male persistently followed her, constantly on the wing and in a state of high agitation. Finally he knocked her to the ground, where he followed her still, and fell beside her, but he seemed exhausted and did not try to mate, but lay as if spent. The female, when she found she had exhausted him, arose on the wing and flew lightly and indifferently away.

A pair in copulo were quietly at rest on a low shrub. I picked them up in the fingers for examination and soon replaced them, but after such disturbance they separated and went flying and fluttering about each other, over the clay bank, over the stream, over the low plants for about seven minutes, when they settled for an instant and I thought they would reunite. But the female dropped several inches to a lower stratum of leaves, remained there for a few seconds and then darted away. I expected the male to go in hot pursuit; instead, for the next ten minutes, while the female was dancing over some shrubs a hundred yards away this male was frantically going in and out among the leaves in the spot on the lower stratum where the female had paused for a few seconds. It was pathetic to see his eager search; the only spot that seemed to have an attraction for him was the place where the female had been, even though he had not seen her drop to it, but had only located the spot after she had left it. As the male hunted frantically about her former location, while the female was dancing in full view, I could not help but feel convinced that in this case at least the sense of sight was inferior to the sense of smell.